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## THE NEW THEORIES OF ECONOMICS.

SEVERAL criticisms of a work <sup>1</sup> that I have recently published have shown me that certain points of view from which the new economic theories can be considered have not been thoroughly understood. In this paper I propose to give a brief exposé of these theories.

The present study is purely objective. It is not my intention to point out the part played by the various economists in the formulation of the new theories, nor have I attempted to separate the large amount of work done by my predecessors from my own modest contribution. If I sometimes speak in my own name, I do so merely to avoid placing the responsibility on other writers for errors I may have introduced into their theories.

As a matter of fact, art has always preceded science. When in the course of the evolution of human knowledge art and science have drifted apart, critics have never been wanting who were ready to assert that science was productive of no useful results. Criticisms of this kind are largely founded on the fact that a science has not nearly so immediate a utility as the cognate art. It is also to be said that art cannot confine itself to its teaching function ; it must also demonstrate its persuasive power. Consequently art is obliged to make use of certain rhetorical devices with which science has nothing to do. As the most persuasive reasoning is not always the soundest, it happens that economic science often differs from economic art in the means of expression employed. Science considers means of expression solely from the point of view of their power to disclose the truth, whereas art must primarily consider their efficiency as means of persuasion. From this it follows that economic science will not hesitate to use mathematics, philology, physiology, etc.; whereas art can draw upon these sciences to but a very

<sup>1</sup> *Cours d'économie politique* ; Lusanne, F. Rouge, 1896, 2 vols.

limited extent for fear of not being understood by the majority of those it undertakes to persuade.

It is not my intention at this point to consider the general question of the utility of the pure sciences. In passing it may be said by way of illustration that any person who believes in the uselessness of the science of astronomy will act prudently in not reading a book the title of which shows that it treats of this subject. On the other hand, if he enjoys reading works on this science it would be foolish for him to stop at every page to express the wish that the author had written a more valuable work, *e. g.*, a trade almanac. It is also to be thoroughly understood that a question may legitimately be entirely ignored or wholly put aside, but if it is considered at all it must be treated as fully as the resources of the science at the time permit.

There can be no doubt that the exact and real sense of the famous phrase of Tacitus : *Agri pro numero cultorum ab universis in vices occupantur, quos mox inter se secundum dignationem partiuntur* . . . has nothing to do with the present welfare of humanity. A statesman having in hand the passage of a law affecting the landed property of his country might therefore be justly condemned were he to waste his time in trying to find out the meaning of this phrase. Any person who might have the time and curiosity to know exactly what had been the system of property among the Germans must needs read the principal authors who have studied this famous phrase of Tacitus. After having once taken up the task it would, however, be very annoying to be interrupted in one's study by the remark of an intruder to this effect : "You are wasting your time. You use too much Latin ; you make a mistake in quoting Greek so often, for many cannot read that language. The ancient Germans and their land system are not in my line." To the last remark I can only reply that if you are not interested in a certain line of work you would do better not to interfere but let those proceed without interruption who are interested in the particular subject and who will take the pains to investigate it in every way calculated to lead them to the truth.

To acquire wealth may be pleasanter than only to know the manner in which it is distributed. For my part, were I of the opinion that a certain book would contribute more than any other to establish free trade in the world at large I would not hesitate an instant to give myself up heart and soul to the study of this particular work, putting aside for the time all study of pure science. The man in whose power it might be to find out the means of alleviating the sufferings of the poor would have done a far greater deed than the one who contents himself solely with knowing the exact numbers of poor and wealthy people in society. However, if anyone wished to pursue such a line of study, he should be left at liberty to use such means as may enable him to discover the law according to which incomes vary.

In speaking of a mathematical formula that gives an approximate expression of this law, Professor Charles Gide says that "the latter is endowed with no power, either of intimidating or discouraging any one of those who aspire to a greater equality of social conditions;" which is equivalent to saying that astronomical studies will not have the power "of intimidating or discouraging" lovers in the choice of a star of their own. The investigator who is in search of the law of the distribution of wealth does not aim to encourage or dissuade anyone; he is simply intent on discovering the truth. This does not imply that some questions are not of greater importance from a strictly scientific view than others. Thus mechanics affords us the means of studying the movements of celestial bodies which attract each other according to a given law, but it is obvious that in this study special attention will center on the movements which take place in so far as bodies are subject to attraction under this law and that no time will be wasted in exhaustive study of other laws that have nothing to do with the matter in hand.

Let us therefore put aside, once for all, discussions regarding the greater or less utility of science or its power of persuading anyone to choose for himself between alternatives, and let us

direct our attention solely to the laws governing certain phenomena. A reader who may have no interest in following a discussion on this line may as well discontinue his reading. Voltaire tells how Micromegas descended to earth and observed mankind. Micromegas, before he could fully comprehend human actions, would have to perceive that a large class of activities aims at providing what is useful or pleasant, that another class of actions depends on the sentiment arising from the mutual attraction between the sexes, and that there is a third class which does not aim directly at the enjoyments of sense. This third class of actions proceeds on certain sentiments called moral, religious, patriotic, etc. Micromegas, if he is to make progress in the study of this great number of actions going on before him, must soon see the necessity of classifying the activities and examining each class separately. This ordering of things is needful in every line of study. It would not do to blend the study of anatomy with that of chemistry. This does not in the least imply that the anatomist underrates chemistry, or *vice versa*; it only says that but one subject can be advantageously investigated at a time.

Economic questions up to this time have too often been construed to coincide with questions of law. The time has now come to separate the two, just as in its time a separation was made between chemistry and physics. What are the economic results of strikes? This is a question distinct from the other question as to whether laborers have or have not the right to organize strikes. Nothing is gained, from a scientific point of view, in not separating these two very different questions. It must also be well understood that in dealing with a practical case both questions must be solved. Science proceeds by analysis, whereas synthesis is required where practice is concerned.

Propositions that are exclusively scientific are of two kinds: (1) descriptive propositions, describing what has taken place—so monetary systems are described with reference to time and place; (2) hypothetical propositions, which search into what would have happened under given specific conditions—so, for

instance, what would happen if paper money were issued in a country accustomed to a circulation of gold.

These two kinds of propositions fairly correspond to those which are in mechanics included under the two heads of actual and virtual movements. Given a system of material points, the problem is to find the actual movements of these points. This is a question following under the first head. Of the second kind would be the question as to what movements these points would describe on the basis of the relations actually existing between them. These latter movements, not realized in fact, but which might have taken place without violating the conditions assumed, are what are called virtual movements.

In my opinion both political economy and social science have in view the study of two analogous kinds of movements. The first kind constitutes the doctrine of evolution; it is the class of movements to which social science has given the greater attention. This class of movements is characteristic of the studies which bear the name "historical" in political economy. The second class of movements or questions is of interest as bearing on the problem what relation given specific facts bear to the welfare of humanity; in other words, what are the potential movements conducive to the maximum welfare of humanity? In what relation do they stand to actual movements? These problems can be solved, if at all, through analysis and abstraction. That is to say, an endeavor is to be made to isolate and study the principal phenomenon. Those who censure political economy for pursuing this method seem to be unaware of the fact that all sciences, without exception, must follow this method. Certain critics cry out apodictically against the new theories as being absurd because they attempt to state economic phenomena "in mathematical formulæ." But no such pretentious attempt has been made. To try to state economic phenomena in the shape of mathematical formulæ would be very much like the physicist trying to apply without modification his mathematical formula for the descent of falling bodies in a vacuum to the movement of a feather floating on the wind. These critics may

therefore be told that, far from aiming to express complex phenomena in a simple formula, economists broadly avow that they do not know and will never know the theory of any concrete phenomena in all its details. They are solely acquainted with ideal phenomena which make a continually closer approximation to the concrete cases. Let us take a very simple example—the fall of a heavy body. The complete theory of its movement is unknown. Who, then, can give us the theory of the movement of a falling feather? If we simplify our problem by supposing a heavy body to be falling in a vacuum and attracted by the earth alone, we may infer the well-known theory of falling bodies. We thereupon complicate the problem by introducing the fact of the air's resistance, and in this way arrive at theories approaching more and more closely to reality.

Such are the considerations which lead to the method of successive approximations in political economy. In my *Cours* I have developed this theory more fully, and therefore shall only indicate its nature here.

Rational mechanics gives us a first approximation to the theory of the equilibrium and of the movements of bodies. In the same way the theories of Jevons, Walras, Marshall, Irving Fisher, and others present us with a first approximation to the full theory of economic phenomena. It must be clearly understood that it is only an approximation; it is similar to that just made in the case of the heavy body supposed to fall in a vacuum. Pure economics has no better way of expressing the concrete economic phenomenon than rational mechanics has for representing the concrete mechanical one. It is at this point that there is a place for mathematics. The problem of pure economics bears a striking likeness to that of rational mechanics. Now, in point of empirical fact, men have as yet not succeeded in treating the latter problem without the aid of mathematics. It therefore appears quite legitimate to appeal also to mathematics for assistance in the solution of the economic problem.

Many people think that the advantage arising from the use of mathematics consists in making demonstration more rigorous.

This is an error. A demonstration well constructed by the method of ordinary logic is just as rigorous as one made by the application of that other kind of logic which bears the name of mathematics. The advantage of mathematics lies chiefly in this, that it permits us to treat problems far more complicated than those generally solved by ordinary logic. Most economists insist upon the mutual dependence of different economic phenomena. But a purely verbal recognition of this fact is not all that can be done or all that is required. What we want is to determine, at least approximately, the relations existing between the economic phenomena under discussion and so obtain a clear conception of their interdependence. A system of equations similar to the one used in mechanics to represent the equilibrium and the movement of bodies is afforded by this method of approximation. This representation is, no doubt, in this way approached in a rough way at best, and yet the approximation serves better than nothing. It is better to know that the earth is nearly round than to imagine that it is a flat surface.

Professor Walras' great contribution to economic discussion was his discovery of a general system of equations to express the economic equilibrium. I cannot, for my part, sufficiently admire this portion of his work, but I must add that I entirely disagree with him on what he has to say in his work entitled *Études d'économie sociale*. Professor Walras thinks it possible to draw certain economic deductions from metaphysical principles of jurisprudence. This opinion is worthy of respect, but I am unable to accept it. I am a believer in the efficiency of experimental methods to the exclusion of all others. For me there exist no valuable demonstrations except those that are based on facts. Foremost among the general problems to be solved by mathematics is the question of the determination or indetermination of economic problems. It is well known that wherever the number of conditions (equations) is equal to the number of unknown quantities, a problem is easily solved. This simple consideration helps us instantly to understand why the so-called "laws of maximum prices" contain a logical and formal impossibility.



I have endeavored to extend to dynamic questions the use of the equations given for the static equilibrium. The most accurate description possible of the economic phenomenon is to be reached in this way. Is it not a most remarkable fact that a system of equations should thus be able to express not only the general character of economic phenomena, but every single detail as far as we may have any knowledge of them. The entire body of economic theory is henceforth bound together in this way and knitted into an integral whole. If our equations are constructed each for a homogeneous group, and several of these groups are considered, we get the theory of non-competing groups of Cairns and an effectively complete theory of international trade, together with an adequate scientific interpretation of the theory of comparative cost. By the use of these general formulas we find the coefficients of production. A consideration of their variability leads us up to a recognition of the function of the entrepreneurs and to the theory of different systems of production according as we postulate either free competition or monopoly or a socialistic régime. It is to be borne in mind that consumers are reckoning in terms of marginal utility or "ophelimity," while the entrepreneur is counting in terms of money. Some of the criticisms that I have seen might have been unnecessary if account had been taken of this distinction. A consideration of the size of industrial enterprises leads us to recognize that there exists in general a definite *maximum* at which the expansion of enterprises stops under a régime of free competition, there being no advantage in increasing them beyond or leaving them short of what corresponds to this magnitude. Our general formula also directs our attention to the distribution of savings among the different uses to which they can be put. From a study of the degree of facility<sup>1</sup> with which savings can be turned

<sup>1</sup> I am not at all satisfied with this way of presenting my idea; but to give it more precision, without entering into a lengthy explanation, would be quite impossible without making use of mathematical symbols. The desire to keep mathematical symbols out of the text explains why in my *Cours* the theory of rent appears in the notes. It was impossible to give it the desired rigorousness and preciseness without the employment of mathematical symbols.

into different species of capital a rigorous theory of rent can be deduced. The general equations show us, besides, how the economic aggregate is thrown into vibration under the action of forces which stimulate it. The study of these vibrations leads to a theory of crises. When once the economic aggregate has in this way been already apprehended it is easy to understand that the divisions into which we break up the aggregate in studying separately exchange, production, and capitalization are quite arbitrary, although they may be useful helps in study. In reality these three operations are simultaneous. If this be so it becomes absolutely necessary to consider them again as a whole after having examined them in isolation—to make a synthesis after having analyzed the phenomena. This general conception of economic aggregate finds an analogy in the conception which an astronomer has of the solar system when he has apprehended the general equations of mechanics.

It is to be borne in mind that, since any economic phenomenon requires time for its completion, the amount of goods under consideration in any given case is the quantity which is produced and consumed in a given period of time. Besides this the use of capital could not be conceived apart from the time relation. Professor Irving Fisher has developed these considerations with much ability.

The general equations of the economic equilibrium are obtained on the hypothesis of a decreasing marginal utility, and it therefore becomes necessary to ask whether these equations yield results which are borne out by experience. This, indeed, is the only adequate demonstration of the propositions that political economy claims as its own and makes the subject of its discussion. The first part of the argument is of use only as drawing the conclusions from certain hypotheses, and says nothing as to their validity as statements of concrete facts. To test their validity in this respect deductions must be compared with experience; (1) because this affords the definitive proof of the hypotheses already made, which until this test be made remain probable only—*a priori*; and (2) because it affords a means of

testing whether this means of presenting economic phenomena really exhibits the substantial bearing of the phenomena in question.

So, for instance, experience teaches us in a general way that the quantity of goods demanded by consumers decreases with an increase in their price. Do or do not the general equations of the economic equilibrium lead us to this conclusion on theoretical grounds? If the question is answered in the affirmative the ground for accepting the general equations is strengthened, whilst if it is answered in the negative the general equations in question must be rejected. Many of the critics who have commented upon my *Cours* have not understood this to be the case; they very erroneously imagined that when thus comparing my deductions with facts I intended thereby to prove the latter. But a fact cannot be demonstrated—it must be observed; it can then be used to test the validity of a deduction. Even the simplest and most elementary facts are to be made use of in making these verifications. From a failure to appreciate this rule of procedure critics have been led to make such reflections on my work as the following:

“After a long deduction from a formula he states that the truthfulness of that which is deduced can be seen at once from simpler considerations. Such overfondness for mathematical machinery strengthens the impression that exists among many that this machinery is capable only of grinding out truisms.”

Remarks of exactly similar tenor might be made upon the discussion of light-interference on the basis of the well-known equations of light-vibrations. But the theory of undulations does not and cannot demonstrate the phenomena of interference; on the contrary it is on the ground of these phenomena that the theory is accepted. The general equations of the economic equilibrium are not of avail for demonstrating the law of supply and demand; on the contrary, the equations which represent the chief phenomena of supply and demand are proven by experience. There is no truism in all this. The accuracy and therefore the value of a calculating machine might well be

tested by an actual operation, *e. g.*, multiplying 15 by 12 and comparing the product with the result obtained by the usual method. But this verification must not be considered a demonstration, and it must not be said that an expensive and complicated machine has been constructed for the sole purpose of multiplying 15 by 12. Another writer, Mr. H. L. Moore, repeats the foregoing objection<sup>1</sup> in nearly the same words. He says: "After making the complex development, he points out that substantially the same results might be obtained without the use of differential calculus. Professor Pareto cannot expect to reconcile his non-mathematical reader with this method of confusing him by telling him (§ 56) that if he does not understand the mathematical explanations he will have all the advantage in learning the language in which they are given!" Mr. Moore would convey the impression to his readers that the observation he quotes concerns this truism; but this is not the case. My observation concern *those results* that cannot be reached *otherwise*, while the inference which can be made "*without the use of differential calculus*" is a simple verification. Similar verifications are to be found in any treatise upon mechanics; but that such may be the case Mr. Moore does not seem to realize. If the rebuke administered by this acute critic is merited in the case just cited, it is also merited in numerous other cases which seem to have escaped him, for the instances of such resort to verification in my *Cours* are numerous. Now it is precisely because the general formulæ of economic equilibrium cover a large number of economic questions which can be demonstrated in other ways, often very simple, that we take these formulæ to represent this whole range of the economic phenomena.<sup>2</sup>

<sup>1</sup> *Annals of the American Academy*, May 1897.

<sup>2</sup> The same critic says: "*A curious slip for a mathematic economist is made in the discussion of population*, a slip that parallels the error of Malthus." To substantiate this he isolates a phrase which he finds in the text—where there are no mathematics—and gives it to the reader under the impression that to prove that the progress of wealth in England has not followed the law laid down by Malthus I furnish but this single ground of inference, *viz.*, that between two given epochs the growth of wealth

For a like reason I have used every occasion to verify my theoretical deductions by statistics and history on this subject; J. S. Mill's work on logic contains several pages which it might be worth the while for some of our critics to reread.

In the early stages of every science there is apt to be a good deal of reasoning about terms rather than about the things themselves. This method of procedure, however, is not entirely erroneous. Words are often the depositories of the experience of men, and so long as a new-born science has not succeeded in accumulating for itself a sufficient aggregate of direct experience it may find it advantageous to have recourse to the common fund of experience more or less vaguely represented by words. There comes a time, however, in the development of science at which the fund formed by direct experience becomes sufficient and at which the disadvantages attached to the vagueness of experience, such as is given us by everyday words, outweighs the advantages to be derived from them. This is precisely the state of affairs in political economy. I believe this

has been more rapid than that of population. He then argues at length to show that one can always make out an arithmetical progression in such form that its terms, within given limits, will be greater than those of a given geometrical progression. Without giving in detail "the complex development" of my critic's argument, it may be characterized as equivalent to the proposition that it is always possible to show that within given limits the ordinates of a straight line will be larger than those of a given curve. The reader who may not be satisfied with Mr. Moore's assertion on this point, and who may be willing to take the trouble to verify the case by reference to my *Cours*, will find (Vol. I, page 341) the following expression for the gross income in England:

$$R = 346.30 \times 100.01104^t$$

The following words, which explain the phrase isolated by Mr. Moore, should also be read: "On voit que la raison de la progression est beaucoup plus rapide que celle qui a été trouvée (211<sup>r</sup>) pour l'augmentation de la population. C'est ce qui explique comment la richesse par tête d'habitant a augmenté considérablement."

According'y, in the text I have observed that in England wealth has grown more rapidly than population, and in the notes I have furnished the precise expression for the *geometrical progression* which has been followed by the growth of wealth. Is it not "a curious slip" for a critic not to have seen this? The reason why the explanation has to be sought for in the notes is simply this,—it could not be given without the use of mathematics and I desired to keep mathematics out of the text. In conclusion it may be said that it should not be deemed unnecessary to read the book one attempts to criticise.

science would gain a great advantage by developing a terminology of its own, just as chemistry, physics, and anatomy have done. But in this transition period many persons judge of works which are written according to the new methods as if they had been written according to the principles which have prevailed in the past. As a consequence, criticisms which I consider of absolutely no value have been made upon the terminology which I have chosen to employ in my *Cours*. For instance, it has seemed to me to be convenient to adopt Professor Walras' definition of capital; I have not, however, deduced any conclusions from it. If anyone should prefer a different definition, there would be no objection to his using it, and he would obtain the same results as I have if he reasoned exclusively about things and not about words alone. I should even venture so far as to say that I could rewrite the whole of my *Cours*, and obtain the same results, without even mentioning the term "capital," if it were worth the while to attempt the feat. In fact, the base of calculations as regards production is the budget of the producer. Here is a person who owned a ship which cost him £2000 on the first of January 1895; its value decreased to £1800 by the first of January 1896. During the year 1895 the person in question expended £1000 and received for the hire of his vessel £4100. These facts must be gathered from his budget, whatever be the form in which it appears. The equations of production in reality only formulate facts of a similar character. But among the various forms which a balance sheet can assume, there is one which is particularly suitable for use in certain theoretical deductions. Let us suppose that we are putting down the figures of our balance sheet and that we put the vessel down for £2000, the value it had on the first of January 1895. As the ship is now worth only £1800 we are obliged to charge the sum of £200 to expenses so as not to violate the form of the budget. Now, since we know that the vessel has the same value on the first of January 1895 as on the first of January of the following year, we may just as well eliminate it altogether from the balance.

The vessel figures in our business only because it has rendered us a certain amount of "services" during the year 1895.

The method of bookkeeping we used for the vessel may be used in the case of many other things. As it is convenient to designate these things by a name, let us call them "capitals." And let us refrain from thinking that in giving them a name we change their nature, and let us at all events not waste our time in discussing the name to be given them. If anyone prefers a different name from the one we have selected, he is free to adopt it. If it should be found convenient to state the budget in some other form than that adopted, we need not hesitate to do so, as the results will doubtless be the same. The very same thing can be considered as capital or not as capital, according to our way of looking at it when we draw up the balance sheet of production. To be capital is not to possess an objective quality analogous to specific weight, chemical constitution, etc.; it is a characteristic which depends upon the way we use a thing in the process of production.

Another perfectly valueless discussion is the one on the question as to whether it is preferable to use the term "factors of production" or the term "coefficients of industry." Let us suppose that four hectares are needed to produce eighty hectolitres of corn. These four hectares are called "factors of production" of *the* corn. Professor Walras takes as a premise in his discussion the quantity of ground needed to give a unit of produce. In the case assumed four-eightieths or one-twentieth of a hectare is required to raise one hectolitre of corn. Professor Walras gives to this one-twentieth of a hectare the name "coefficient of industry." Now, does it not amount to the same thing to say that four hectares of ground yield eighty hectolitres of corn, or that the one-twentieth part of a hectare yields one hectolitre of corn? One must be overfond of disputing about words, to let oneself drift into endless discussions as to the best term for presenting any given fact. We might mention a great many other discussions of the same character—all similarly wanting in simple common sense. But I prefer to drop this

subject and to pass on to the consideration of more serious questions.

The presentation of the economic phenomenon in its entirety affords a clear comprehension of the effects of all conceivable economic organizations concerned ; but it must be borne in mind that this knowledge will always remain approximate only. Science does not attempt to establish any particular method of economic organization, and it is not the business of science to do so. Science does, however, attempt to solve problems of the following kind : (1) What are the effects of a régime of free competition ? (2) What are those of a régime of monopoly ? (3) Those of a collectivist régime ? All these questions must, of course, be treated, not from a polemical point of view, but solely for the purpose of ascertaining what results would follow upon their installation. It is especially necessary for us to discover what relation these results bear to the aggregate well being of humanity ; and to do this not only a first approximation, but a second and a third, and perhaps even more, must be made, because the later approximations take account of secondary facts which are easily neglected in the earlier ones. The régimes compared may appear to yield identical results if only a first approximation be undertaken, and may differ materially in their results when reconsidered for the purpose of a second approximation.

By following this line we have been able vigorously to prove that the coefficients of production are determined by the entrepreneurs in a régime of free competition precisely in the same way as a socialist government would have to fix them if it wanted to realize a maximum of ophelimity for its subjects. This proposition is one of the main points of the so-called classical political economy ; but it was necessary to give this proposition precision, in order to see clearly the limits within which it holds true and what conditions it assumes as given. This was also necessary in order to obtain a general proposition showing that every indirect transfer of wealth from one individual to another is accompanied by a loss of wealth. As a result of this it appears that if a socialist government is to obtain a max-



imum of well being for its subjects it must modify the distribution of wealth only in a direct way—say by taxing some people for the benefit of others, or by some similar means. A second approximation will take account of the expense of putting the mechanism of free competition into full play, and will compare this expense with that necessary for establishing some other new mechanism which society may wish to test. This is the way in which an engineer would first compare two systems of machinery from a theoretic point of view, keeping to the principles of thermodynamics for steam engines; then he would make tests of friction, loss of steam power, etc., in each system.

This method of approaching the subject differs substantially from that adopted by a large class of economists, who, after giving in their adhesion to a system, put forth all their power in showing its advantages and in defending it against all attacks to which it may be exposed. I once more repeat that I am far from condemning such economists, for I regard their work as one of great utility. This being the situation, I have no desire to offer a substitute for their work, but simply to add to it a purely scientific study. All the conclusions to which deductive studies founded on the general equations of the economic equilibrium can lead us must finally be verified by a careful scrutiny of facts, both present and past—that is to say, by statistics, by close observation, and by the evidence of history. This is the method of all the material sciences. Deductive studies in political economy must not be opposed to the inductive; these two lines of work should, on the contrary, supplement each other, and neither should be neglected.

The incidence of duties is a special case of such alterations as the economic equilibrium undergoes when certain of its conditions are modified. The new theories give us an exact idea of the nature of the incidence of duties. In order to ascertain this with exactness we must have at hand the numerical data bearing on this incidence; but statistics have not as yet supplied them. This delays the practical application. A great advance toward the solution of the problem has been made, however, when the

method has been found by which the problem is to be solved. So, for instance, if we wish to know the area of a rectangle two things are required—(1) the knowledge that the area is to be obtained by multiplying the width by the length and (2) the exact measurements of the width and the length. Geometry gives us only the former item.

The laws of the distribution of wealth evidently depend on the nature of man and on the economic organization of society. We might derive these laws by deductive reasoning, taking as a starting point the data of the nature of man and of the economic organization of society. Will this work sometime be completed? I cannot say; but at present it is certain that we lack sufficient data for undertaking it. At present the phenomena must be considered synthetically, and every endeavor must be made to discover if the distribution of wealth presents any uniformity at all. Fortunately the figures representing the distribution of wealth group themselves according to a very simple law, which I have been enabled to deduce from unquestioned statistical data.<sup>1</sup> This law being empirical, it may not always remain true, especially not for all mankind. At present, however, the statistics which we have present no exceptions to the law; it may therefore provisionally be accepted as universal. But exceptions may be found, and I should not be greatly surprised if some day a well-authenticated exception were discovered.

Meanwhile, this law as it stands has most important theoretical results, and these resulting formulæ may be employed without limitations of time and place, provided the law be verified by statistics.

<sup>1</sup> This law is as follows:  $N = \frac{A}{(x + b)^a}$ . In which  $N$  represents the number of individuals having an income greater than  $x$  or  $A$ ;  $b$  is a constant which for aggregate incomes is in general zero, or very near it;  $a$  is another constant whose value lies between 1 and 2. The law applies only to incomes a little above the minimum. The form of the curve in the immediate neighborhood of this minimum income is still undetermined, for statistics do not furnish us sufficient information for its determination. Since the publication of my *Cours* I have examined many new statistical data, and they all verify the law which I had there formulated. The results of my later investigations have been published in the *Giornale degli Economisti* (Rome).

Mathematics is employed in connection with this law, but in a totally different way from that in which has been utilized in the analytical studies considered at the outset. In those studies the complexity of the phenomena compelled us to make use of mathematics; in the case we have just been considering they present us under the forms of a simple curve the entire sum of results supplied by statistics. But in the one case, as in the other, those who are not well at home in the use of mathematics can nevertheless master the results gained through their use. In my *Cours* it has been possible to throw the mathematical parts into footnotes, and the text can thus be read without hindrance by persons who have no knowledge of mathematics.

This law of the distribution of wealth, which has so lately been discovered, may some day be of use in the study of the different races of men in this respect.<sup>1</sup> The application of the law is certainly conditioned by the heterogeneity of the population. It does not in any way prejudice other theories of the distribution of wealth, for the law is merely the synthesis of numberless statistical facts. It can be compared in some respects to Kepler's law in astronomy; we still lack a theory that may make this law of distribution rational in the way in which the theory of universal gravitation has made Kepler's law rational.

VILFREDO PARETO.

UNIVERSITY OF LAUSANNE.

<sup>1</sup> Mr. Carlos C. Closson has made an important contribution to this line of study in this JOURNAL for September 1896.